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Perspectives

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A Centennial: George W. Beadle, 1903–1989

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George Beadle was a quadruple-threat man – scientist, teacher, administrator, and public citizen. In each of these he excelled. Furthermore, he did what very few geneticists did in those days. He studied three different organisms and made outstanding discoveries with each. He followed his interests and undertook his diverse responsibilities with zeal, confidence, and serenity.

In October there was a centennial symposium at the California Institute of Technology at which several of Beadle's colleagues spoke. Here we present a biography followed by brief vignettes from several of his associates. Some are taken from talks given at the symposium.

A BRIEF BIOGRAPHY (JFC)

Beets (as he was known to his friends) was born October 22, 1903, on a farm near a Nebraska town with the wonderful name, Wahoo. He loved rural life and expected to be a farmer. He never abandoned these roots and he maintained a lifetime interest in agriculture. No matter how busy his life, he almost always had a garden. At the urging by a high school teacher, who realized his promise, he entered the University of Nebraska in 1922. He graduated in 1926 and, encouraged by his major professor, F. D. Keim, stayed another year. At that time he was interested in ecology and conservation and his first publication dealt with the ecology of hay grasses.

Maize. In 1927 Beadle entered the graduate school at Cornell University, where he was exposed to the excitement of genetics. He soon joined the maize genetics laboratory of R. A. Emerson and there enjoyed the stimulating company of fellow graduate students Marcus Rhoades, Charles Burnham, and Barbara McClintock. He was immediately productive and wrote 14 papers based on work

done at Cornell. Several of these dealt with mutants affecting synapsis, meiosis, or disjunction. He was interested in crossing over and later coauthored a classical paper on linkage studies and chromosome mapping (Emerson, Fraser and Beadle, 1935). At that time, the density of the linkage map of maize was second only to that of *Drosophila*. Noteworthy because of Beadle's later return to this subject, were several papers on maize, its wild relative, showing their great chromosomal similarity and intercrossability. Beadle was convinced that teosinte was the ancestor of domesticated maize.

Drosophila. After receiving his Ph.D. in 1931 Beadle received a National Research Council fellowship to work at CalTech. There he worked in E. G. Anderson's cornfield and lived close by. He used this period to complete several studies started at Cornell. Soon, however, he caught the excitement of Morgan's fly group and, moving closer to the CalTech campus, started working on *Drosophila*. His early work was concerned with crossing over and, with A. H. Sturtevant, the effects of chromosomal inversions on crossing over. One of his clever experiments employed heterozygous attached X chromosomes to demonstrate the four-strand nature of crossing over and the absence of chromatid interference. This was an experimental tour de force, since attached X chromosomes tend to become homozygous. Beadle had to use triploids to introduce markers into the attached X chromosomes and create heterozygotes. Later he joined with Sturtevant to prepare a textbook that set a new standard of clarity and rigor (Sturtevant and Beadle 1939).

Having made major contributions to the cytogenetics of both maize and *Drosophila*, Beadle took off in a totally new direction. All along he had had an interest in gene action and he saw an opportunity in Sturtevant's discovery that the eye pigment at the *vermilion* locus develops non-autonomously in mosaic flies. Specifically, the *vermilion* mutant eye develops the wild-type pigment if there is wild-type tissue elsewhere in the fly. This provided a golden opportunity. Waiting for suitable mosaics was uncertain, so he tried a new, adventurous approach. He joined his friend Boris Ephrussi in Paris to do a series of experiment in which eye primordia were transplanted from one larva to another.

After some initial difficulties the experiments finally worked, and were a great success. Beadle and Ephrussi were able to find that the normal alleles of *vermilion* and *cinnabar* mutants, v^+ and cn^+ , act sequentially. The v^+ and cn^+ substances, acting as hormones, opened up the opportunity for their identification. Meanwhile, Beadle returned to the United States to join the faculty at Harvard. He was not happy there and after a year accepted a position at Stanford. There, he was joined by Edward L. Tatum, who brought chemical skills to the project. They soon found that the sought-after substances were derivatives of tryptophan.

Eventually Tatum obtained crystals and by standard chemical methods identified the ν^+ and cn^+ substances as kynurenine and OH-kynurenine. But much to his and Beadle's disappointment a German group headed by Adolf Butenandt had identified them first. Butenandt had simply tested various known derivatives of tryptophan. Why Tatum didn't try this simpler approach isn't clear.

Neurospora. Beadle and Tatum now recognized the limitations of *Drosophila* for further biochemical work. Pondering this, Beadle had a magnificent insight. While still a graduate student, he had learned of Dodge's work on the bread mold *Neurospora*. More recently at CalTech, Lindegren had worked out its meiotic details. *Neurospora* was made to order for genetic research. It had the convenient properties of being haploid and producing meiotic spores in linear order in an ascus. Furthermore it was able to grow on a minimal medium with only a carbon source and biotin. Beadle reasoned that he could produce mutants lacking in the ability to synthesize a nutrient, and identify them by their ability to grow on complete but not on minimal medium. The 299th spore had the requisite property. It was a straightforward process to find the specific requirement, which turned out to be pyridoxine. Soon after, mutants requiring thiamine and *p*-amino benzoic acid were discovered and the floodgates were open. Gene control over enzymes had long been realized by Garrod, Cuenot, Haldane and Wright, but Beadle's technique delivered enzymatic deficiencies in wholesale quantities. It marked the beginning of biochemical genetics as a systematic study. Tatum later found that the same methods could be used for *E. coli*, and this led to his discovery with Lederberg of a sexual process. It was a major step in what Muller (1947) called "The Coming Chemical Attack on the Gene" and what Warren Weaver termed "molecular biology".

Beadle also proposed the one-gene one-enzyme theory. Although exceptions became increasingly common, it was highly influential in the thinking of the time. Furthermore this work attracted biochemists to the field. There were immediate practical applications, such as bioassay and production of more efficient penicillin producers. Beadle's lab was an exciting place.

CalTech. In 1946 Beadle returned to CalTech to replace Morgan as Chairman of the Biology Division. Building on the already strong genetics program, he recruited a stellar group of faculty colleagues. From Stanford he brought Norman Horowitz and David Bonner. He soon added Herschel and Mary Mitchell, Max Delbrück, Ray Owen, Renato Dulbecco, and Robert Sinsheimer. As the division grew, Beadle found that the job of running a department was a big one and encroached on his research time. He soon became a full time administrator. His combination of informality, openness, and strength of leadership made him an outstanding chairman.

By 1958, Beadle had built up the program in Pasadena to become a world center for molecular biology. Much, probably most of his time was going to obligations outside of CalTech. The Division was going beautifully. He decided to take a sabbatical and accept an offer to spend a year at Oxford. There he taught courses and absorbed the British culture. It was during this period that Beadle and Tatum shared the Nobel Prize with Joshua Lederberg. Further honors, such as honorary degrees, poured in. Eventually, he received a total of 37 honorary degrees, 11 major awards, and 15 honorary society memberships. For a complete list as well as a selected bibliography, see Horowitz (1990).

Public Service. Beadle was increasingly active in other ways. He regarded public education in genetics as a personal responsibility and gave frequent speeches before diverse groups. He was also highly successful as a fund-raiser for CalTech biology. At the same time he became increasingly active in national committees. An increasing fraction of his time was spent on airplanes, where much of his writing was done.

He was on the original committee that recommended genetic studies in Hiroshima and Nagasaki. He was able to attend a bomb test in Nevada. Although he was scrupulously careful with the nation's military secrets, he was active in protecting scientists who were regarded as security risks, including his CalTech colleague, Linus Pauling. Beadle became an advisor to the Biology Division of the Atomic Energy Commission. In 1954 he was President-elect of the AAAS and took a strong stand against excessive secrecy. He was a member of the National Academy committee on the Biological Effects of Atomic Radiation (BEAR), which issued an influential report in 1956. After that, he became chairman and a second report was issued in 1960. Beadle showed his diplomatic skills in dealing with an appendix written by Wright, with which Muller vehemently disagreed. Beadle also served as chairman of the scientific council of the American Cancer Society. He was instrumental in getting the Society to adopt a broader view of what could be studied under the rubric of cancer research.

Chicago. In 1961 Beadle surprised everyone by accepting the presidency of the University of Chicago. This came as a particularly great shock to his CalTech associates. Although Beadle's name had been mentioned more than once in connection with university presidencies, his decision to go to Chicago was made without his CalTech colleagues knowing what was happening.

Why did he do it? For one thing, Beadle had always been restless, never staying very long at the same place. For another, this was a challenge, and Beadle liked a challenge. The University of Chicago felt that it was losing academic

standing. It was losing faculty and it needed more money. Also there were serious problems of student unrest and urban renewal in the University area.

It was a wise decision on the part of the University. Beadle increased the faculty numbers by 25 percent during his tenure and he improved the standing markedly. By his retirement, he had doubled the budget and had achieved his aim of raising \$160 million. His ability to glide serenely through the most contentious academic and community problems are part of the Beadle legend. Especially, he built up biology and medicine, and several of the departments achieved national recognition. And, as was his lifetime custom, he had a garden.

Retirement. At age 65 Beadle reached the mandatory retirement age and was succeeded by the man, Edward Levi, who as Dean of the Law School had been most effective in persuading Beadle to accept the Chicago challenge. Beadle stayed in Chicago, but he didn't stop working. He held a couple of short jobs, but mainly he returned to his first love, the origin of maize.

As a graduate student, he knew that maize is closely related to teosinte, both genetically and cytologically.. He and Emerson thought that teosinte was the most likely ancestor of maize. During his busy years of research and administration, he had no time to pursue this interest. But he became increasingly annoyed by a theory proposed by Paul Mangelsdorf. Mangelsdorf thought that maize was derived from an extinct or unknown ancestor, and that by its crossbreeding with *tripsacum*, maize was derived. Teosinte, he thought, came later.

Beadle was convinced that this was dead wrong and took time off to argue his case for teosinte (Beadle 1939). After retirement, he did a number of genetic experiments and became convinced that a rather small number of mutations could convert teosinte into a reasonable facsimile of maize. He also showed that the hard kernels of teosinte could "pop", just like popcorn, and suggested that this provided an incentive for early farmers to cultivate it. He not only did small-scale experiments in his garden, but he organized expeditions to Central America. In one experiment he examined 2 million seeds. There were several group meetings during the "corn wars" in which he and Mangelsdorf debated. In the end, Beadle provided the most convincing evidence and, as later shown by Doebley, he was right in almost all particulars.

Beadle's personal life. While at Cornell Beadle met and in 1928 married Marion Hill, a graduate student in botany. They worked together on some experiments. A son, David, was born in 1931. Later, they were divorced and in 1935 Beadle married a writer, Muriel Barnett. Among other things she wrote informative and amusing books about their stay in Britain and the Chicago years

(Beadle, M 19—, 1972). The book on Britain has the wonderful title, "These Ruins are Inhabited."

In the second decade after his retirement Beadle began experiencing memory loss. He stayed in Chicago and continued his experiments as long as he could, but finally returned to California. At age 80 he was diagnosed as having Alzheimer's disease and died on June 9, 1989. Muriel survived him by five years.

For a book-length biography of Beadle, see Berg and Singer (2003)

SOME VIGNETTES

Following are several short items, reflecting views of those who knew Beadle in one way or another. Several are extracts from talks given at CalTech in October, celebrating the centennial of Beadle's birth.

MEMORIES OF A COLLEAGUE (NH)

It is a pleasure for me to take part in this symposium of distinguished scientists assembled to celebrate the centennial of my old friend George Beadle. It was in this room, 62 years ago, that Beadle presented the results of the experiments that he and Tatum were then carrying out at Stanford University with the red bread mold *Neurospora*. These experiments founded the science of what Beadle and Tatum called "biochemical genetics." In actuality, they proved to be the opening gun in what became molecular genetics and all the developments that have followed from that.

I'd like to try to convey something of the mood of that seminar. The year was 1941. It was the Golden Age of classical genetics. T. H. Morgan was still active as first Chairman of the Biology Division. The room was full. Beadle was well known at Caltech. He had been a post-doc here and had co-authored a book with Sturtevant (STURTEVANT and BEADLE 1939). Beadle was introduced, and he presented the astonishing results of the revolutionary paper that was then in press in PNAS (BEADLE and TATUM 1941). He described the first three nutritional mutants of *Neurospora*, mutants with requirements for pyridoxin, thiamin, and p-aminobenzoic acid, respectively. He spoke for just 30 minutes and then sat down. There was no applause, because the audience could not believe that someone with such findings could talk about them for just 30 minutes. We had never heard such experimental results before. It was the fulfillment of a dream, the demonstration

that genes had an ascertainable role in biochemistry. We were all waiting—or perhaps hoping—for him to continue. When it became clear that he actually was finished, the applause was deafening. Prof. Frits Went, whose father had done the first nutritional experiments with *Neurospora* in Java early in the century, stood up and turned to a group of graduate students sitting in the audience and said "You see—biology is not a finished subject—there are still great discoveries to be made!"

A few years later, Beadle summarized the *Neurospora* findings with the statement "One gene, one enzyme," or more broadly, "one gene determines the structure of one protein." This was the keystone of molecular genetics.

The full breadth of Beadle and Tatum's discovery soon became clear. They had shown that a class of lethal mutants new to genetics—mutants requiring essential substances, such as vitamins, amino acids, purines, pyrimidines, or their precursors—could be recovered by working with a microorganism that could be grown on a synthetic medium. In the case of *Neurospora*, the medium was a simple inorganic one containing sugar and biotin. In short, they had demonstrated that genes have an essential role in biosyntheses. At the time, some nongeneticists still believed that genes governed only trivial biological traits, such as eye color and bristle pattern, while basic biochemistry was determined in the cytoplasm by an unknown mechanism. Many respected geneticists believed that gene action was far too complex to be resolved by any simple experiment. In other words, Beadle and Tatum had brought about a real revolution.

THE ONE-GENE ONE-ENZYME THEORY IN RETROSPECT (PB)

Formulated in the mid-1940s, the one gene-one enzyme maxim gave physical meaning and function to genes. While imprecise in detail, the notion that each gene specifies a unique enzyme (protein) was, in its time, a "bombshell". As with most concepts in biology, time and the advancing knowledge modified the original one-gene-one enzyme proposal. First, came the change to one gene-one protein to be replaced by one gene-one polypeptide as it became clear that many proteins contain more than one polypeptide chain, each specified by a different gene. Then we learned, particularly in eukaryotes, that one gene often gives rise to more than one polypeptide as a consequence of the organization of genes into polypeptide-encoding exons and non-coding introns and the phenomenon of alternative splicing. And some genes do not encode a polypeptide at all but rather encode functional RNA molecules.

In ascribing an instructional role to genes, Beadle and Tatum implicitly accorded genes an informational property. This insight provided a foundation for a genetic code. Admittedly, not until Avery's and Hershey's proof that DNA was the genetic material, Sanger's demonstration that proteins have a defined linear array of amino acids and Watson–Crick's solution of the DNA structure, was there a basis for thinking about a genetic code. In his seminal paper on the "Sequence Hypothesis", Crick conceded that the one gene–one protein axiom made it likely that the linear array of base pairs in DNA specified the linear array of amino acids in the protein.

Until Beadle and Tatum's experiments with *Neurospora*, mutations were used primarily as genetic markers for studying the mechanisms of inheritance. Geneticists relied on spontaneous, random events that altered an observable or measurable property of an organism. There was no need to know the function of the mutated gene to determine its linkage to other genes or to follow its inheritance from one generation to the next. But with more efficient means for making mutations, Beadle and Tatum's experimental paradigm provided a way to analyze metabolic pathways. Soon afterward it became the preferred way to dissect complex biological systems, for example, embryonic development, cell division, the nature of sensory systems, aging etc. Today, mutational analysis is the preferred way into a complex biological problem, especially as it provides access to the genes and protein players.

Beadle was an early articulate spokesman for the integration of biochemistry and genetics. Although not a biochemist, he assailed the barriers between biochemists and geneticists. He was prophetic in believing that the biochemist could not understand what goes on chemically in the organism without considering genes any more than the geneticist could fully appreciate the gene without taking account what it is and what it does. He coined and consistently used the term biochemical genetics seemingly preferring it to molecular biology although the latter won out in the end.

The flowering of molecular biology and the emergence of genomics has occupied center stage during the past fifty years and in that time produced its own set of "heroes". Beadle was never a direct participant in the explosive advances that followed the solution of the DNA structure. His modest demeanor and 'nice guy, straight arrow' manner would likely have put him at odds with the flamboyant and quirky behavior of several of the major players of the new biology. They likely viewed him as "a non-combatant" figure from the past.

BEADLE AS A PUBLIC CITIZEN (MS)

(Forthcoing)

BEADLE AND TATUM (JL)

The "one gene: one enzyme theory", namely that a gene acts by controlling the formation of a specific enzyme in some fairly simple manner, was already implicit in the research on pigment biosynthesis. Its evolution can be related to the broad reach of mechanistically oriented research in biology. Haldane's speculative discussion is closely parallel; but the theory was never so concretely asserted, nor used to plan such effective lines of enquiry, before 1941. The *Neurospora* work suggested that any biochemical trait could be readily studied in like fashion; Beadle and Tatum plausibly extrapolated from several diverse examples that all such traits would have an equally direct relationship to corresponding genes. This generalization is now rephrased in the terms that the DNA sequence provides the information for protein structure. The numerics might sometimes be more complex: many genes might be involved in the quantitative regulation and environmental responsiveness of enzyme synthesis, and sometimes of families of enzymes. Enzymes are sometimes complex multi-chain ensembles, or may contain non-protein cofactors, requiring the participation of many genes. The role of RNA as a message intermediate between DNA and protein, the complexities of intervening sequences in RNA, RNA-processing, and post-translational processing were future developments requiring more sophisticated biochemical analysis, but all were inspired by and made great use of the concepts that were concretized by, and of the tools generated from, the *Neurospora* studies.

Their essential contribution then comprised several parts:

- 1) A methodology for the investigation of gene-enzyme relationships exploiting experimentally acquired genetic mutations affecting specific biosynthetic steps.

2) A conceptual framework, the one gene: one enzyme theory, that provided a context for the search for and characterization of these mutants, and reflected back to a primary model that the chromosomal genes contained (substantially) all of the blueprints of development, and that enzymes (and other proteins) were the mediators of gene action.

3) The [temporary, as it turned out] dethronement of *Drosophila* as the prime experimental material for physiological genetic research, in favor of a fungus, *Neurospora* — which helped open the way to bacteria, viruses, and tissue cells cultured as if they were microbes. (LEDERBERG 1988)

MEMORIES OF A STUDENT (MS)

I met George Beadle in 1957, when I arrived at Caltech as a new graduate student in the Division of Biology. I had been offered a teaching assistantship to support my first year of study at Caltech, but the offer did not specify the course in which I would be doing my teaching. As it turned out, I was assigned to the team of teaching assistants in Bio 1, the introductory biology course in which George Beadle and James Bonner were the lecturers. I had, of course, heard of both Beadle and Bonner before I arrived at Caltech, and I was pleased to be given an assignment that would give me an opportunity to get to know them.

Beadle was a wonderful lecturer, clear and well organized, and students could easily see that he was fascinated by the enormous breadth and complexity of biology. The job of the TA's was simply to oversee the labs in the course and to try to convey in the lab the same sort of enthusiasm that Beadle conveyed in the lectures. Beadle's frequent and cheerful interactions with the TA's were helpful. In our weekly discussions of the upcoming labs, he was always able to point out the elements in the lab that the students would find most interesting and provocative. And I particularly remember grading sessions in which we all sat in one room and struggled to evaluate short essay answers that were often quirky or syntactically bewildering. These sessions were both hilarious and instructive, thanks to Beadle's happy acceptance of the oddities of undergraduates and his long experience in making sense of their rambling prose.

I want to mention Beadle's introduction to his lecture on evolution,

because it differed so dramatically from the introduction that I had heard when I was a freshman biology student. My professor at Washington University had started the lecture by saying that the Missouri Synod of the Lutheran Church, to which many of the students in the class belonged, rejected the idea of evolution. "When the church and science are in conflict on some issue," said the professor, "the church had better back down." This statement made a good many students extremely unhappy. Beadle, on the other hand, introduced the subject by saying that some religious people were uncomfortable with the theory of evolution, but he didn't see why. If you wanted to think of God as the creator of all living things, what would be wrong with thinking that God used evolution as the mechanism? It was clear that Beadle was no more religious than my professor at Washington University, but he was certainly more diplomatic.

The TA's were so comfortable with Beadle that we called him "Beets." I don't think he ever invited us to use his nickname, but he was so egalitarian in his dealings with us that it seemed natural to address him as his faculty colleagues did. Beadle was chair of the Division of Biology at the time, and, of course, the Division of Biology at Caltech was a collection of all-stars, among whom Beadle sparkled as brightly as any. There was, however, no hint of pomposity in Beadle.

Beadle invited the TA's in Bio 1 to his house for dinner. That evening remains vivid in my memory. Beadle and his wife, Muriel, lived in the Morgan house, which had been the residence of Thomas Hunt Morgan, and Beadle clearly considered it to be hallowed ground. He told us a bit about Morgan's contributions to genetics just to make sure that we appreciated the significance of being in Morgan's house. We met Beadle's cats, and he told us about their breeding. Beadle liked Siamese cats, but disapproved of their raucous voices. His cats were the result of his own breeding experiments to discover the genetic basis of the annoying voice and to produce Siamese cats with mellifluous meows. Beadle grilled the steaks for dinner and instructed us in how to prepare them. You started with the best top round steak from Jorgensen's Market and marinated it in a mixture whose ingredients he listed for us. He had us gather around the grill so that we could learn how to prepare the charcoal and turn out a perfect round steak. Beadle showed us a beautifully illustrated book on the mountains of Alaska, in which he was identified as a member of the first team to climb Mount Doonerak. He was as proud of climbing that mountain, he said, as he was of anything he had ever done. Beadle told us that he had taken up rock climbing around the age of 50 and that, while he was still a novice, he had made a challenging climb with a group of students, all of whom were much younger. When they got to the top, one of his fellow climbers, a medical student, suggested that, since they had all just entrusted their lives to one another, it might

be nice to learn each other's names. On hearing Beadle's name, the student asked, "You are not by chance the Beadle of Beadle-and-Tatum?" Beadle admitted that he was. The student said, "Man, I thought you were dead!" Beadle was clearly delighted by this story.

I have one other personal recollection of Beadle that demonstrates something of his leadership style. My wife gave birth to our first son in the spring of my first year as a graduate student. Our health insurance adequately covered the costs of the hospital, but not the bill from the obstetrician. The bill was so much larger than we had anticipated that I decided I would have to drop out of graduate school and get a job. I made an appointment with Beadle to ask whether Caltech would be willing to hold a place for me so that I could return to graduate studies after I had paid off our debt. Beadle was outraged at the size of the obstetrician's bill. He asked the doctor's name and phoned him immediately. "What do you think you're doing, man?" he asked the obstetrician. "This kid's a Caltech graduate student. He can't afford to pay medical bills like that." The doctor promptly reduced his bill, and I was able to continue my graduate studies.

THE ORIGIN OF MAIZE (JD)

(forthcoming)

SOME PERSONAL MEMORIES (JFC)

In 1956 I attended an International Symposium on Genetics, held in Japan. It was an exciting time for those of us – most of us – who had never been to Japan. J. B. S. Haldane was scheduled to give the opening plenary address. He was at his boorish worst. First, he refused to stay in the accommodations that had provided for him. Then, despite heroic efforts on the part of the organizers, Haldane was not satisfied with the speaking arrangements and refused to give the opening address. Beadle was scheduled to give the closing address. Always the gentleman, he agreed to give his lecture on the opening day. By the end of the Symposium some days later Haldane was satisfied and give his talk. That wasn't all that Beadle did, for as Chairman of the BEAR committee he spent many hours with Daigoro Moriwaki, his counterpart in Japan.

Sometime, probably in 1958, Beadle visited the University of Wisconsin. R. A. Brink had arranged for an informal gathering with light refreshments. He

then asked casually of Beadle whether anything of interest had happened at CalTech recently. Beadle replied that the answer was yes. So we all went into a small lecture room and Beets proceeded to give a wonderfully lucid lecture on the Meselson–Stahl experiment. I think it was totally new to everyone. We were all impressed by the beauty of the work, but also by Beadle's crystal clear, off the cuff description of it. I vividly remember how excited I was to hear about what has been called "the most beautiful experiment in biology".

My first chance to get well acquainted with Beets was on the BEAR Committee. This committee was chaired by Warren Weaver, who although a mathematician turned out to be a very quick study of genetics. The committee, as so often happens, got bogged down in an argument, not about what we should conclude, but how we should explain it. Wright and Muller were completely at odds and for a while it looked as if we wouldn't have a report. Wright had written a statement, actually quite a thorough analysis as only Wright would do, but Muller thought that it would dilute the impact of the report. It was finally agreed not to include Wright's statement, probably the result of Beadle's diplomacy. The report was widely publicized and set the tone for radiation protection standards from that time on. Beadle negotiated to have Wright's lengthy statement included with the 1950 report. Wright was not entirely happy with the postponement, for he knew that any later report would be largely ignored – which was the case. It was finally included in the 1950 report at the time when Beadle was Chairman.

At about this time Beadle, as was always true, was involved in all sorts of activities. In particular, he was on a committee to oversee a series of science programs on national television sponsored by the Bell Telephone Company. Beadle naturally arranged to have one of these on genetics and he got me to be the scientific consultant. The film was made in the Warner Brothers Studio in California. It was my first (and last) experience with the Hollywood culture. Beets got Norman Horowitz to work with me. This turned out to be more work than I had expected and involved several trips to California. What made them most pleasant, however, was that each time I was a houseguest of Beets and Muriel, gracious hosts both.

While Beadle was still at CalTech, the University of Wisconsin was looking for a president. A leading candidate was Lee DuBridge, who promptly turned it down. All along, Beadle had also been considered. A group of Wisconsin Committee members went to CalTech to interview Beadle and this drew from DuBridge the comment "Don't these people know that I have said no." It turned out that a local candidate was named. I don't know whether Beadle would have accepted. Not long after, he went to Chicago.